ESTIMATES AND CAUSES OF CRUSTAL SHORTENING.[1]

INTRODUCTORY.

IN THE following paper I shall use the words crust and nucleus as terms by which to conveniently refer to the outer known solid shell of the earth, of which we have direct knowledge, and to the core surrounded by the crust, of which we have only inferred knowledge. The use of these terms in this sense is independent of any hypothesis as to a sharp boundary between the two, and of any theory as to the condition of the interior of the earth. So far as my present purposes are concerned, the nucleus may be entirely liquid, entirely solid, part liquid and part solid, or in a state of matter of which we have no knowledge.

The intricate phenomena of earth deformation, and particularly that form of deformation called folding, has led geologists to assume, in order to account for the facts in the field, that the surface of the earth must have been vastly shortened during geological time. Some instances of the estimates of the amount of crustal shortening may be mentioned.

Dutton[2] thinks, to explain the phenomena of folding since the close of the Cretaceous, that the radius of the earth must have been shortened more than thirty miles. He states that the plications are so great that we must assume a contraction on some circles of latitude since the commencement of the Permian amounting to many hundreds of miles, and this amount of contraction is small, he says, compared with that involved in the Laurentian rocks. Heim[3] estimates the transverse shortening of

[1] Published by permission of the Director of the U. S. Geological Survey.

[2] A criticism upon the contractional hypothesis, by C. E. DUTTON: Am. Journ. Sci., Vol. VIII, 1874, p. 121.

[3] Mechanismus der Gebirgsbildung, von ALBERT HEIM: Basel, Band II, 1878, S. 213.

the Alps to be seventy-four miles. Le Conte[1] estimates the transverse shortening of the Coast Ranges of California to be from nine to twelve miles. Claypole[2] estimates the transverse shortening of the Appalachians in Pennsylvania to be forty-six miles. McConnell[3] thinks the folding of the Laramie range of British America required a transverse shortening of twenty-five miles.

In the above estimates of shortening by Heim, Claypole, Le Conte, and McConnell, I have inserted the word *transverse*, to call attention to the fact that shortening in only one direction has been considered by these authors. It is clear that to obtain an adequate idea of the effect of crustal corrugations, it is necessary to know in square miles the surficial lessening of the crust of the earth as a result of deformation. However, it appears that if in other mountain ranges the shortening is proportional to the estimates above given, the total amount of surficial decrease must be enormous. This would be true even if the deformation of ancient mountain ranges, the stumps of which are buried under later rocks, were ignored. Moreover, it is possible that the amount of folding and consequent shortening of the Paleozoic and older rocks, buried under the Mesozoic and Cenozoic strata, may be as great or even greater than the amount of shortening involved in the deformation observable at the surface.

The theory of mountain-making as a result of secular cooling has been repeatedly attacked along the lines of the vast contraction demanded by the supposed facts of the field, and the small contraction resulting from secular cooling, the only cause ordinarily assigned for contraction. Dutton[4] calculates that the

[1] On the structure and origin of mountains, with special reference to recent objections to the contractional theory, by JOSEPH LE CONTE: Am. Journ. Sci., Vol. XVI, 1878, p. 98.

[2] Pennsylvania before and after the elevation of the Appalachian mountains, by E. W. CLAYPOLE: Brit. Assoc. Rept., Montreal meeting, 1884, p. 718.

[3] Geological features of a portion of the Rocky mountains, by R. G. MCCONNELL: Geol. Surv. of Can., Ann. Rept., Vol. II, Pt. D, 1886, p. 31.

[4] Loc. cit., p. 120.

contraction due to secular cooling is mainly confined to the outer 200 or 300 miles of the earth, and states: "Although no estimate can be made of the contraction of this portion, it is probably safe to say that its volume cannot have been diminished so much as one-tenth; and if we were to assign thirty miles as the diminution of the earth's radius since the formation of a cooled exterior, we should probably reach the utmost limits consistent with Fourier's theorem."

It is believed, upon the one hand, that there may have been great overestimates of the amount of crustal shortening, and upon the other hand, that important causes for nucleal contraction may exist which have not been sufficiently considered. It is the purpose of this paper (1) to examine the evidence upon which estimates of crustal shortening have been made, and to consider the methods to be followed in making estimates of shortening, and (2) to summarize the known causes which may exist for nucleal contraction and crustal corrugation. The paper may thus be divided into two parts. In Part I, I shall consider the shortening of the outer surface of the earth accompanying folding, faulting, jointing, cleavage, fissility, and vulcanism and cementation; and in Part II, I shall consider the causes which may account for the shortening represented by the phenomena.

PART I. ESTIMATES OF CRUSTAL SHORTENING.

Folding.—The deformation of folding undoubtedly involves shortening, but it is believed that it does not necessarily require nearly so much shortening as has been believed. Estimates of shortening resulting from folding have not considered the effects of the following phenomena: (1) the thinning of the layers produced by folding; (2) the composite character of folds and the rapid variations in the closeness of the folds of the various orders; and (3) the effect of gliding on the limbs of folds.

1. In order to make an estimate of the amount of shortening involved in folding, it is necessary to recall the nature of the deformation of the individual beds and formations. It has been

shown[1] in another place that the mashing, flowage, and the shearing motion involved in differential movement between the layers necessarily involves thinning of the limbs of the folds or thickening of the troughs and crests, or both. Even where the

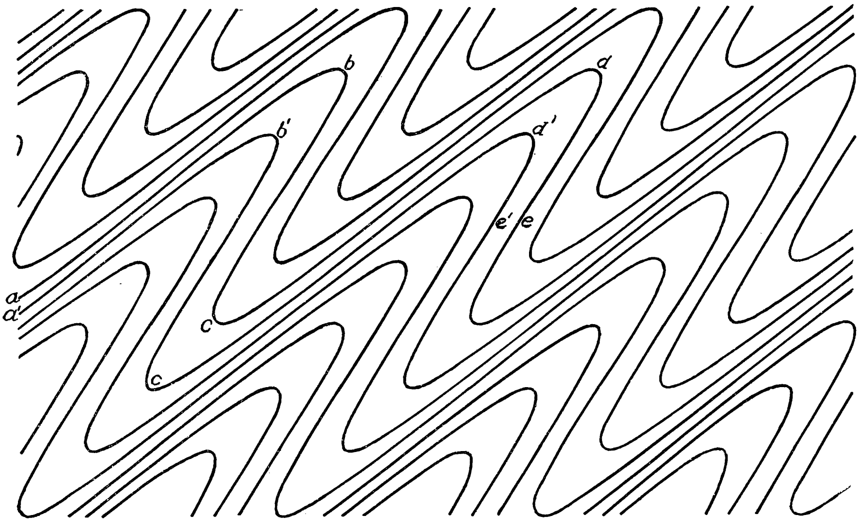

FIG. 1.— Similar monoclinal folds.

folds are not close, in case the folds are similar,[2] the limbs may not be much more than half as thick as are the troughs and crests (Fig. 4). This distortion becomes more and more important as the folding becomes closer, and in isoclinal and monoclinal folds, in which the strata turn back upon themselves, the amount of thinning of the limbs or thickening of the troughs and crests is very great (Fig. 1). A layer when traced out in such a set of folds alternately thins and thickens, and the section if developed on a plane, would alternately greatly widen and narrow (Fig. 2). The length of the developed layer should be the length of its central part. The excess of material for each

[1] Principles of North American pre-Cambrian geology, by C. R. VAN HISE: 16th Ann. Rept. U. S. Geol. Survey, Pt. I, 1896, p. 599.

[2] Loc. cit., pp. 599–600.

curve on the convex side of such a central line would equal the deficiency on the concave side, and consequently such a developed layer truly represents its mass and average length. If it be assumed that the original horizontal stratum had a

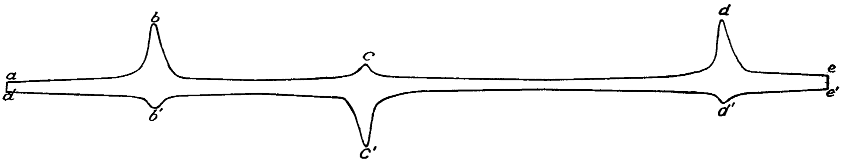

FIG. 2.— Development of a part of a layer of Fig. 1.

thickness equal to the thicker parts of the folded stratum, it would follow that the developed stratum is much longer than it was originally. However, probably in no case is this assumption wholly correct, and in many cases it is far from the truth. The weaker and stronger layers require consideration separately.

At most places the evidence seems to show that during deformation the anticlines and synclines of the weaker layers have been thickened. However, since during the deformation there is shearing motion under pressure all along the limbs, it can hardly be doubted that in many cases the thinning of the layers is a more important phenomenon so far as their length is concerned, than thickening on the anticlines and synclines.

In the stronger layers, often no evidence of thickening is anywhere seen. Upon the contrary in many cases, as will be shown below, the layers are actually elongated by tensile fracture upon their convex sides and therefore cannot have been thickened. Radial open fractures upon anticlines and synclines, due to tension, are beautifully exhibited in the southern Appalachians (Fig. 8). The openings in many cases have been filled with quartz. These joints are evidences of tensile forces. Where stretching of anticlines and synclines occurs in the zone of flowage, this is undoubtedly due to the great friction between the layers, and to their positions on the convex sides of the neutral planes. The limbs of the stronger layers, like those of the

weaker beds, are subjected to differential shearing under pressure, and if distortion occurs they must be thereby thinned.

The actual thinning and elongation of layers as a result of folding has been noted by Le Conte[1] as an important phenomenon in the Coast Ranges of California, and by Reade[2] in various mountain ranges.

According to Gilbert,[3] during the introduction of igneous rocks, which formed the Henry mountains, the pressure of the magma normal to the strata was so great that they were extended laterally by flowage a sufficient amount to cover the domes. In the case of the Holme's arch the linear extension was about 2 per cent.

The amount of thickening or thinning, which any given formation or layer undergoes, will of course depend upon many factors, among which attitude, strength, pressure, amount of differential movement or shearing are to be considered.

As noted by Reade,[4] the attitude of the layers is of the greatest importance. In their initial position the tendency of the pressure is to thicken them. This tendency continues as the layers are tilted, until the average dip is 45°. As soon as the layers upon the average have a greater inclination than 45° (Fig. 1), the average effect of the tangential pressure is unquestionably to thin the layers, although some members at certain places, and especially at the turns, may be thickened. When it is remembered that in the closely-folded mountains the layers generally have dips greater than 45°, and as explained later (pp. 16–17) such layers usually turn quickly at the anticlines and synclines, it becomes evident that the thinning of the layers and their consequent elongation, as a result of tangential pres-

[1] On the structure and origin of mountains, with special reference to recent objections to the contractional theory, by JOSEPH LE CONTE: Am. Journ. Sci., Vol. XVI, 1878, pp. 299, 301, 302.

[2] The origin of mountain ranges, by T. MELLARD READE: London, 1886, pp. 176, 208, 211.

[3] Geology of the Henry mountains, by G. K. GILBERT: Rept. U. S. Geog. and Geol. Surv. of the Rocky mountain region, 1877, pp. 80–82.

[4] Loc. cit., pp. 216–217.

sure in positions which average greater than 45°, may be very important. In reference to the other factors, the greater the pressure, the greater the tendency for thickening and shortening at angles less than 45°, and the greater the tendency for thinning

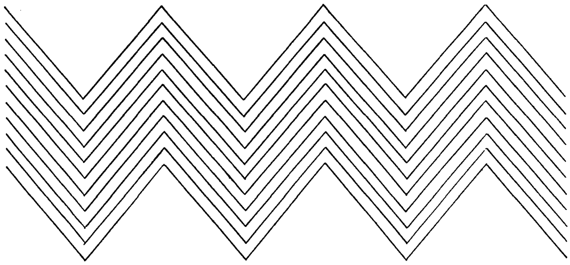

FIG. 3.— Similar upright folds with angular crests and troughs.

and lengthening at angles greater than 45°. The greater the shearing between the layers, the greater the thinning. The greater the rigidity of any given layer, the less the thinning.

The foregoing statements are understood to apply to strata which are so deeply buried that the deformation of the layers results from true interior distortion of them. In the upper zone of fracture these statements need to be modified, as subsequently explained.

With present data only these qualitative statements can be made. But, it seems probable that in the earlier stages of the development of folds, the average thrust thickens instead of thins the layers. However, where the folds are very close, and especially in isoclinal and overturned folds, it can hardly be doubted that upon the average there is considerable thinning and consequent important elongation of the layers. For folds of a given average closeness the average amount of distortion is not so great where the strata bend back upon themselves with sudden turns, as where the bends occur gradually (compare Figs. 3 and 4), although the distortion at the angles may be greater than at the corresponding places upon the gentle turns. In nature the deformation is ordinarily between the two extremes figured.

The fact that the distortion is less in folds with sudden turns than in those with rounded turns, is believed to be a cause why this deformation so frequently occurs in closely-folded rocks. The angular deformation requires less work, and there-

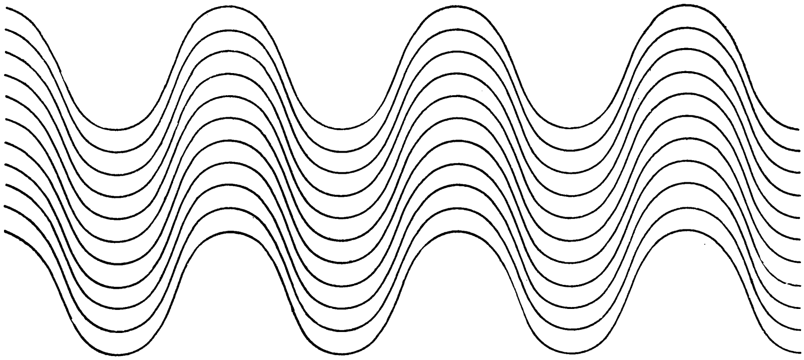

FIG. 4.— Similar upright folds with rounded crests and troughs.

fore less energy than the other form. As the stresses slowly accumulate until the elastic limits of the rocks are surpassed, the deformation which will occur in any given case in order to relieve the stresses is that which requires the least expenditure of energy under the existing conditions. This is the old principle that a mass under stress gives way along the lines of least resistance.

2. In estimating the amount of superficial shortening involved in folding, it is necessary to consider what and where strata shall be selected for estimation. I have shown that if there is no thinning or thickening of the layers (Fig. 5) folds rapidly die out above and below any given folded layer. I have also shown that similar folds are only possible by profound distortion of the layers (Figs. 1 and 4), unless the turns are very abrupt. Agreeing with theory, actual geological sections are a compromise between parallel folds and similar folds, the folds rapidly varying in closeness in different parts of a mountain mass, vertically and laterally.[1]

[1] Principles of North American pre-Cambrian geology, by C. R. VAN HISE: 16th Ann. Rept. U. S. Geol. Surv., Pt. I, 1896, pp. 599–601.

In this connection it should be remembered, if the theory of a level of no lateral stress be true, that a good reason exists for the lessening folding and distortion of layers with increasing depth.

Whether or not this theory as ordinarily stated approximates

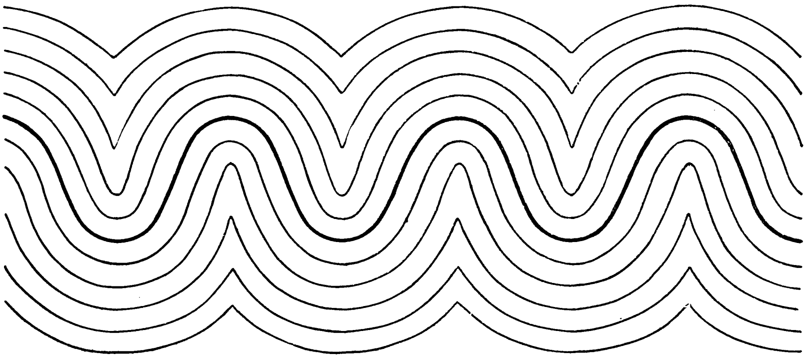

FIG. 5.— Parallel upright folds with rounded crests and troughs.

quantitative correctness, it is certain that the amount of shortening must somewhere decrease with increase of depth; for infinitesimally near the center of the earth the amount of shortening must be infinitesimally small. Since with present knowledge we can only conjecture the law under which folds die out in depth, though we are certain that they must die out, one is not justified in assuming that folds similar to those at the surface continue even for moderate depths.

If this principle be ignored in estimating shortening, a serious error may be made. The formation being followed may plunge beneath softer formations which show close plications. If it be assumed that similar plications also effect the formation below to be measured, this may lead to a considerable overestimate of the amount of crustal shortening (Fig. 6).

Also the lateral variation in closeness of folding may lead to error. If the layer or formation to be measured is not continuously exposed, it may be visible where it chances to be most closely folded and be concealed where more openly folded. If at

the places of less folding the strata chance to be hidden, the plications of other strata must be selected to fill in the gap. Layers may be selected which exhibit close folding. But even if layers are selected which show the least folding of any in sight,

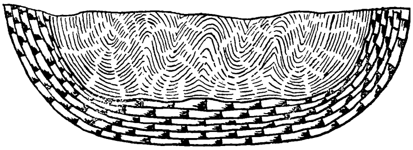

FIG. 6.—Simple fold below composite folds.

there is still a possibility that a considerable overestimate may be made of the closeness of folding and of the amount of crustal shortening.

For oftentimes, where a formation upon which estimates of shortening are being made, disappears below the surface, this results from its plunging downward as a part of a synclinorium. It is believed that upon the average synclinoria are less closely folded than anticlinoria. Anticlinoria are places of crustal thickening resultant upon close plications, whereas synclinoria are areas of depression as compared with the anticlinoria, but not really areas of depression as compared with the unfolded districts. If the plications of synclinoria were as composite as anticlinoria, this would involve an equivalent amount of thickening of the crust, and consequently equal elevation with the anticlinoria unless a large amount of material, to compensate for the difference in elevation, had flowed from below the synclinornia to below the anticlinoria. Doubtless the flowage of the kind suggested does take place to some extent, but to no such extent as would be involved in this hypothesis

Willis's experiments most beautifully illustrate the composite character of the folding of anticlinoria as compared with the intervening synclinoria.[1] The above reasoning applies exactly

[1] The mechanics of Appalachian structure, by BAILEY WILLIS: 13th Ann. Rept., U. S. Geol. Survey, Pt. II, 1893, Pls. LXXVII, LXXXI, LXXXII, LXXXIV, LXXXV, LXXXVI.

to his experiments. When the strata were compressed in these experiments there was flowage from below the synclinoria, but not a sufficient amount to allow the synclinoria to become as plicated as the anticlinoria, and at the same time to be at a lower level.

3. In anticlinal mountain masses the cores, composed of the oldest and originally deepest-buried strata, are often less closely plicated than the strata on the flanks of the mountains, which are composed of younger rocks. In estimating the crustal shortening of such mountain masses, we have, therefore, the

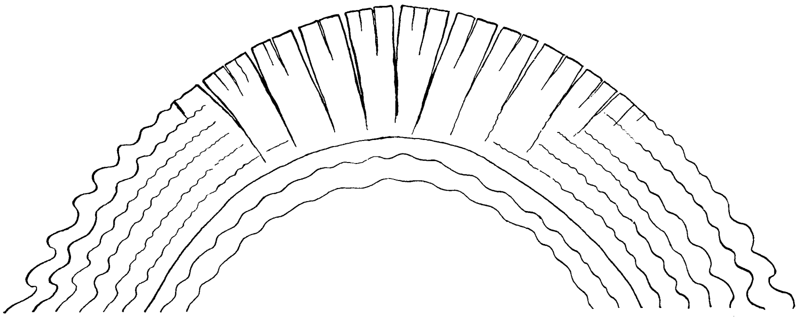

FIG. 7.— Possible relation of secondary folds to joints.

difficulty already mentioned, that is, folds of different layers vary in their closeness. This occurs notwithstanding the fact that upon the limbs one would expect that differential movement between the layers, or shearing, which tends to stretch them rather than to produce plications, are at a maximum. The greater folding of the limbs, as compared with the cores may be partly explained by the principle already given, that in general folds die out as depth increases, and consequently the older strata are least folded. However, the plications are probably to be chiefly explained by the gliding of the material down the slope upon the flanks of the mountain, because under the stress of gravity (Fig. 7). The strata on the crests may have been removed by erosion, or, as explained in another place (see p. 24), the stresses may have there produced joints. In either

case the material on the flanks is no longer held in position by the tensile strength of the rocks on the crest, and under the stress of gravity slides down the slope, and this results in corrugations. The plications upon the flanks may thus be partly or fully compensated by separation of the material along the crests. However, the plications may be inferred, in estimating crustal shortening, to have extended to the part removed by erosion. In this case the amount of crustal shortening due to folding would be overestimated. In reality, the original length of the strata was that of a gentle continuous curve of the order of magnitude of the mountain mass.

The question may be asked as to the reality of the existence of the gliding effect above assumed as a result of the action of gravity. In another place[1] I have fully discussed the forms of the secondary folds which occur in composite anticlinoria and synclinoria of the first order. It there appears that the secondary folds upon the flanks of the mountains so commonly have attitudes which must have resulted from this gliding effect, that the composite folds, the secondary folds of which show such attitudes, have been called normal composite folds. This discussion cannot be here repeated, but if the argument given be correct, the gliding effect due to gravity producing secondary corrugations upon mountain flanks is a significant phenomenon, and consequently the cause here assigned for overestimates of the amount of crustal shortening is of importance.

It is clear that in appealing to the force of gravity to pro duce corrugations upon the slopes of the mountains, I am follow ing Dana[2] and Reyer.[3] However, I do not follow the latter fully. He makes the gliding the cause of the formation of mountains, whereas, it is clearly an effect, following the mountain-making. Material cannot glide down until it has been raised up. My

[1] Principles of North American pre-Cambrian geology, by C. R. VAN HISE: 16th Ann. Rept. U. S. Geol. Surv., Pt. I, 1896, pp. 608–615.

[2] Geological results of the earth's contraction in consequence of cooling, by JAMES D. DANA: Am. Journ. Sci., Vol. III, 1847, p. 185.

[3] Theoretische Geologie, by E. REYER: Stuttgart, 1888, p. 829.

present point is that if the corrugated parts are alone considered, and the jointed part or the part removed by erosion ignored, that the average crustal shortening may be greatly overestimated.

If the considerations presented in the foregoing pages have weight, it is clear that the actual measurements in the field of the amount of crustal shortening involved in folding presents great difficulties, and the question naturally arises as to the best practicable methods of procedure.

1. So far as practicable, the same formation should be measured throughout a section, and if it is necessary to transfer from one formation to another, the greatest care should be exercised in order to avoid the errors which may result from changing from a formation to another lower or higher, and also to avoid the error which may come in as a result of the lateral change in closeness of plication.

2. The strongest formations available should be selected for measurement.

This selection should be made because the stronger formations have less composite curves than the weak ones. As a consequence they are less distorted during the folding than the weak formations. These facts may be observed in almost any good section of closely-folded heterogeneous strata. The more composite crenulation, but not the greater thinning and thickening, of the weaker layers may be illustrated by bending a rectangular pile composed of bunches of paper alternating with cardboards, the pile being held firmly either mechanically or with the fingers at the edges, so that slipping between the laminæ may be hindered at the places held. In this experiment, at the crest or trough, spaces form between the stronger layers, and in these spaces the weaker layers take on secondary crenulations. In natural geological sections the pressure upon the limbs is frequently sufficiently great so that the material of the weaker layers flows toward the openings on the anticlines or synclines, and partly or wholly occupies them. In many places some of the weaker layers are quite pinched out upon the limbs.

The physical cause for the simple folding of strong layers

and the composite folding of the weaker layers is that already assigned for another kind of deformation on p. 17, namely, under given conditions, the deformation occurs which demands the least expenditure of energy. To deform the strong layers in a composite way requires a large amount of energy. To deform the weak layers in a composite way requires much less energy. The simple deformation of the strong layers and the composite deformation of the weak layers demands less energy than would be required to deform all the layers in a similar manner, so that the deformation would average the same as in the case of the unequal deformation of the strong and weak layers.

Under different circumstances the strong layers vary greatly in the simplicity of their deformation. In case the load is not too great, as explained by Willis, the strong layers are bent into large, simple folds. If, upon the other hand, the load is too great for the strata to support, the strong layers are folded in a composite manner. Both of these cases fall under the principle that the deformation occurs which requires a minimum expenditure of energy, for in the case of the lighter load, it requires less energy to elevate the load on the anticline, or to somewhat depress it on the syncline, than it does to greatly distort the strong formations. But in the case of the great load it requires less expenditure of energy to distort the strong layer a sufficient amount to make it develop composite folds than it does to elevate the load. But as above stated, even in the case of composite folding of the stronger layers, the weaker layers adjacent to them show still more composite deformation.

The statement made that the strong formations should be selected for tracing above the surface and for measurement is therefore justified.

3. From the places where the strong formations plunge below the surface to the places where they reappear, only the most gentle curves should be assumed (Fig. 6).

4. From the places where the formation which is being measured is lost because removed by erosion, only the most

gentle practicable curves should be assumed to the places where the formation reappears, and even if this be done, as shown (pp. 20–22) an overestimate may be made of the length of the formation.

5. It should be ascertained whether the formation measured has upon the average been thinned or thickened, and a corresponding allowance should be made.

If the principles are not appreciated upon which the foregoing precautions are based, with the natural, indeed almost inevitable tendency for one to pick out strata for measurement which have suffered severest deformation here, and severest deformation there, we may be sure that estimates of shortening will have comparative little value.

Jointing.—In another place[1] I have explained that joints may be of two kinds, tension joints and compression joints. Tension joints in simple folds may form in one direction at right angles to the bedding, or nearly so, in the zone of fracture (Fig. 8). In the case of complex folding, two sets of tensile joints intersecting each other at right angles may develop, both, however, still normal to the bedding or nearly so. Compression joints, forming in shearing planes, are ordinarily more or less diagonal to bedding. However, the greatest compressive stresses may approximate angles of 45° to the bedding, in which case the shearing fractures would be nearly normal to bedding. Compression joints, like tension joints, may develop in two directions at right angles to each other.

In the gentle folds of the Paleozoic of the Mississippi valley and the strata of the plateau country of the far West, joints are normal to the bedding, or nearly so, corresponding in position to the direction of the folding. For instance, southern Wisconsin is a gentle southward-plunging anticline, in other words, the principal fold has a nearly north-south axis, and the rocks dip east to Lake Michigan and west to the Mississippi river. Corresponding to this arrangement are numerous joints in a north-south direc-

[1] Principles of North American pre-Cambrian geology, by C. R. VAN HISE: 16th Ann. Rept. U. S. Geol. Survey, Pt. I, 1896, pp. 668–672.

tion. However, the plunge of this anticline varies in passing from north to south. In other words, there is here a great but very gentle cross fold, and corresponding to this is another set of joints which run in an east-west direction.

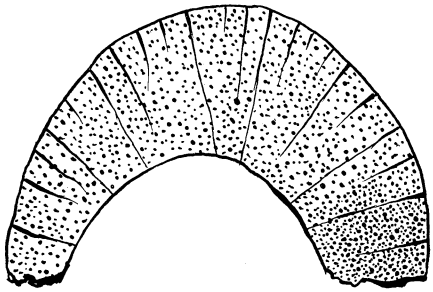

FIG. 8.—Tensile joints.

The same arrangement of joints is still better illustrated in the closely folded Allegheny mountains, and the Coast Ranges of Oregon and California. In the Allegheny mountains, as may be seen by sections along the railroads (for instance, the Pennsylvania, and Baltimore and Ohio) in the stronger beds there are two sets of joints everywhere corresponding to the strike and dip, in other words, corresponding to the two directions of fracture due to longitudinal and transverse folding.

Such joints may be seen both in anticlines and synclines. They occur in sandstone, grit, or limestone. Where the layers are a foot or more in thickness, and the rocks are gently folded, the joints may be several feet apart. Where the layers are closely folded the joints are frequently less than a foot apart. In the thinner layers, those from two to six inches in thickness, the joints are ordinarily less than a foot apart, and where closely folded are but two or three inches apart. Indeed, in some cases of close folding, the two sets of joints are so close together as to break the formations into a set of parallelopiped blocks, the dimension along the bedding being the least of the three, that is, the joints are closer together than the thickness of the beds.

It is apparent that the Paleozoic strata of the Mississippi valley and of the Plateau region, the Alleghenies, and Coast Ranges were folded under such conditions that the curves of the folds were produced not by actual bending of the layers, but

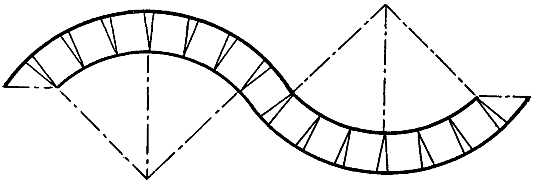

FIG. 9.— Diagram of radial openings produced by tensile fracture.

by numerous fractures, with a slight displacement of each block, resulting in a curved form (Figs. 9 and 10).

Now these joints must have been produced by tensile stresses or by shearing stresses. If they are of the first class, it is self-evident that the production of the joints involved surficial elongation (Fig. 9). If they are of the second class, their production may have involved all of the surficial elongation (Fig. 10), and it will be explained in a subsequent number of this JOURNAL[1] that joints of this kind are believed to be widespread. Some reasons for this belief may here be mentioned. These joints in many regions show a marked tendency to a vertical attitude, as in figure 10. Also the kind of displacements generalized in figure 10 has been observed at various places. Moreover, such joints are closer together the closer the folding, and in some cases they are so close as to make the intervening masses approach leaflets, as, for instance, in sandstones and shales on the Chesapeake and Ohio canal, three miles west of Hancock, Md.

In both the cases of joints produced by tension and shearing above described, there is no real elongation of the strata, but merely a displacement of the blocks causing surficial elongation. In the case of the tension joints this elongation is due to the fact that spaces are measured; in the case of the

[1] Deformation of rocks, by C. R. VAN HISE: JOURN. GEOL., Vol. VI, 1898.

shearing joints the apparent elongation is due to the fact that the measurements are diagonally across the blocks, instead of following the bedding.

From figure 9 it is plain that the average theoretical elongating effect of tension joints is directly as the thickness of the layers or formations through which the joints continuously extend, and indirectly as the radius of curvature. In the field it oftens happens that as a result of the position of a layer or formation upon the convex side of the neutral plane of deformation, the different blocks are separated from one another on the concave sides of the curves as well as on the convex sides.

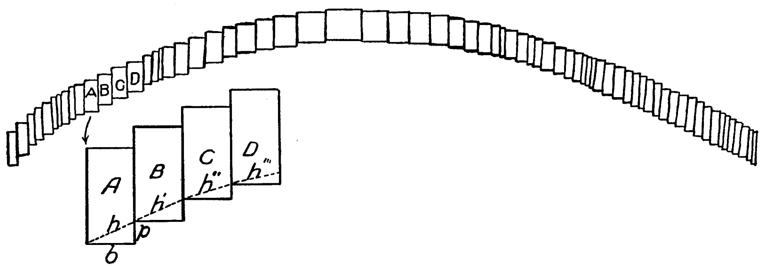

FIG. 10.—Surficial elongation resulting from shearing joints.

From figure 10 it is plain that the surficial elongating effect of the shearing joints is great in proportion to the displacements along the joints, and to their frequency. The apparent length in any case is the sum of the hypotenuses of the right angle triangles ($h+h'+h''+h'''$, etc., Fig. 10), the bases of which are the lengths of the blocks parallel to the bedding, and the perpendiculars of which are the displacements.

It is not to be concluded from these illustrations that there is no crustal shortening as a result of joint folding. Shortening might occur even if the entire bending were accomplished by tensile joint fracturing. Also in the case of the shearing joint fracturing the rubbing of the blocks against one another might produce shortening.

However, if an estimate of the original surface of the layers were made, upon the supposition that it was as great as it would appear to be if developed on a plane, this would result in a

considerable overestimate of the amount of its original surface.

In many of the weaker layers of folded rocks is a diagonal jointing, due to differential motion, and cutting this diagonal jointing nearly at right angles is a diagonal fissility.[1] The whole may result in thinning the limbs of the folds, just as does the shearing motion in the case of folding by plastic flow.

It is very desirable that the quantitative value of the lengthening effect of jointing should be known for various kinds of deformation. This, however, is an exceedingly difficult task. The quantitative value of the surficial elongation due to jointing for any deformation of an area can be only approximately determined after an extensive and close field study of the district. Consequently, for the present, I am obliged to be content with comparative statements which rest upon my own judgment, and which may be questioned by other observers. I believe the elongating effect of jointing to be quantitatively of sufficient importance that it should be taken into account in estimates of crustal shortening. I believe the lengthening effects of joints are important in connection with the estimates of shortening due to folding where the folds on the flanks of mountains may be due to a downward gliding effect, and be compensated by the joints (see Fig. 7), as explained on pages 20–22. However, I suppose that the elongating effect of jointing is not so great as that of the distortion of closely folded rocks in the zone of flow, as explained on page 16.

Faults.— Faults are ordinarily classified into normal faults and reverse faults. The normal faults involve an elongation of the crust of the earth as certainly as the reverse faults involve a shortening of the crust of the earth.[3] The very names, normal and reverse faults, show that the first are of far greater abundance,—are in fact the rule.

[1] Principles of North American pre-Cambrian geology, by C. R. VAN HISE: 16th Ann. Rept. U. S. Geol. Survey, Pt. I, 1896, pp. 651–654.

[2] Supplementary notes on deformation of rocks, by C. R. VAN HISE: JOURN. GEOL., Vol. V, 1897, pp. 190–191.

[3] Principles of North American pre-Cambrian geology, by C. R. VAN HISE: 16th Ann. Rept. U. S. Geol. Survey, Pt. I, 1896, p. 674.

However, since the hade of a reverse fault is usually flatter than that of a normal fault, the shortening due to a given vertical displacement of a reverse fault exceeds the shortening due to the same displacement of a normal fault. In considering the elongation or shortening of the crust of the earth by faults, this factor must be considered, as well as the factor of their relative frequency and area of distribution. However, reverse faults are usually confined to closely-folded areas, while normal faults frequently occur in these same areas, developing in the final stages of deformation, and are also present throughout great regions where reverse faults are absent, as, for instance, in the Great Basin and Plateau regions. It is therefore wholly possible that the amount of shortening of the crust of the earth resulting from reverse faults is more than compensated by elongation of normal faults, and thus the sum total of deformation by faulting result in dilatation rather than shortening of the crust of the earth.

Cleavage.—Cleavage has been supposed necessarily to indicate an important shortening of the crust of the earth.

It is, however, to be remarked that the shortening of cleavage must not be considered if the amount of shortening involved in the folding is counted for the same region; for cleavage is a phenomenon which may result from distortion under conditions of flowage, and the shortening represented by folding includes that involved in the simultaneous production of cleavage.

Moreover, cleavage is possible without any shortening whatever. I have shown in a previous paper[1] that cleavage may be produced by simple shearing motion parallel to the surface of the earth. The inclination of the cleavage will depend upon the amount of the shearing. Shearing of a very moderate amount will produce cleavage with dip as low as 30°. In the production of cleavage by shearing, each individual particle is shortened. Where shearing motion parallel to the surface of the earth results in cleavage inclined at 30°, the amount of shortening of each particle is about .4. However, the direction of shorten-

[1] Deformation of rocks, by C. R. VAN HISE: JOURN. GEOL., Vol. IV, 1896 pp. 636–637, 868–872.

ing is inclined to the surface of the earth. The shortening involves an equivalent elongation in another direction. This elongation is at right angles to the direction of shortening, and is inclined to the surface of the earth in a direction opposite to

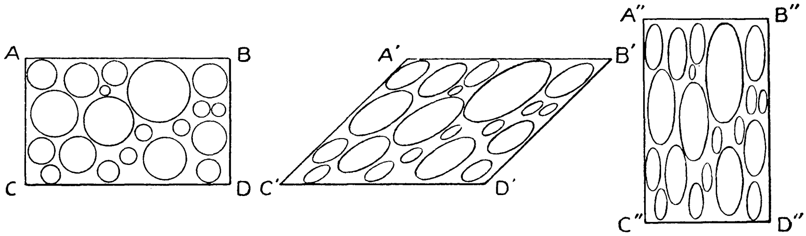

FIG. 11.— Inclined cleavage produced by shearing motion parallel to bedding without crustal shortening, and vertical cleavage produced with crustal shortening.

the direction of shortening. The forces producing a shear involve two couples, which at any given moment produce a tension in the direction of elongation and a compression in the direction of shortening. Thus, as a result of the work of the two couples in the production of cleavage by shearing parallel to the bedding, the direction of tension and the direction of shortening being inclined to the surface of the earth in opposite directions, are in such relations to each other at any given time and place that the total effect is neither elongation nor shortening of the crust of the earth.

This is illustrated by figure 11. The rectangle *ABCD* is deformed into the parallelogram *A′B′C′D′*, by shearing motion parallel to the bedding. The cleavage is parallel to the flattened ellipsoids. The area of the rectangle and parallelogram are the same, and also the line *A′B′* at the surface of the cleaved rock is of the same length as the line *AB* of the original rock before it was deformed and cleavage produced. The Ocoee slates of the Hiwassee river, west of McFarland for several miles, show a cleavage dipping to the southeast at an angle averaging about 30°. The beds are easily recognizable, and are very nearly horizontal. They show no bendings which can be dignified by the name of folds. However, even in this case the

shearing motion parallel to the surface may have been accompanied by nonrotational distortion, as a result of horizontal thrust. The deformation resulting from such stresses is shown by figure 11. In this case the rectangle *ABCD* is deformed into the rectangle $A''B''C''D''$. The shortening is here great, and yet the beds are horizontal, although thickened. In nature the two cases may be combined in any proportion. In the Hiwassee slates already mentioned close observation shows slight crenulations of the generally horizontal strata. These suggest that the shearing motion parallel to the surface has been accompanied by horizontal shortening, and that both kinds of deformation are here combined. But the relative value of each is entirely unknown, and it is therefore impossible to give any estimate of the amount of crustal shortening involved in the deformation which resulted in the cleavage described.

We therefore conclude that while monoclinal cleavage over considerable areas may involve no crustal shortening, it is probable, in most cases of such cleavage, that there is a real crustal shortening, although it is impossible to estimate its amount.

After an inclined cleavage has been produced in any region, the conditions of deformation may change as a result of denudation, and fractures may form parallel to the cleavage. These fractures may be wide apart or close together. After these partings are produced, displacements may occur similar to those of joints (Fig. 10) or they may be closed by the falling down of the overhanging material, precisely the same as in the case of ordinary normal faults. The possible elongation resulting from these secondary movements may partly or fully compensate for the earlier movements resulting in shortening.

Fissility.— Fissility is a name applied to an actual close parting of a rock which results in the production of laminæ. Fissility may possibly develop as an independent structure, although it is believed that it is commonly a structure secondary to cleavage. It is further thought that fissility generally forms as the result of ruptures along shearing planes parallel to the cleavage, from compressive rather than tensile stresses. Where

these ruptures occur close together, and there is slight differential movement, a distributive displacement may be produced, which is equivalent to a reverse fault, and which therefore results in crustal shortening, or the distributive displacements may be similar to those of the shearing joints of figure 10 and therefore result in crustal elongation. However, as in the case of cleavage just described, a region which is under compressive tangential stresses, and therefore is deformed by distributive faulting parallel to fissility, may later be under conditions of tensile tangential stresses. In this case partings will occur between the fissile laminæ, and elongation result. These elongations are strictly analogous to the elongation of normal faulting. The openings may be closed, as in the case of normal faulting, by a dropping down of the overhanging strata, or by methods of injection or cementation, as explained below.

Minute normal fault slips, secondary to cleavage or fissility, have been observed in the crystalline rocks near Blowing Rock, N. C. While during the formation of cleavage or fissility it is probable that shortening took place, it cannot be asserted that the subsequent elongation did not compensate for this, and it cannot be ascertained whether the total effect of the various deformations in this district resulted in elongation or shortening.

Vulcanism and cementation.— After a secondary structure has been produced, whether it be cleavage, fissility, joint, fault, or irregular structure, it may be taken advantage of by igneous intrusions in connection with deformation. These injections with the assistance of orogenic movements may greatly widen the openings so as to make places for great dikes. Such injections result in the local elongation of the crust of the earth. The injections may be divided into two classes, regular, approximately parallel injections, which take advantage of the above regular structures, and irregular injections.

Throughout considerable districts the amount of parallel injected materials is equal to, or surpasses the amount of original materials in which the regular secondary structures were

produced, and thus there are large extensions of the areas affected.

This is finely illustrated by many districts of the Piedmont plateau crystalline and semicrystalline rocks. A convenient district in which to see the phenomena is that of New York. In the Manhattan gneiss in the vicinity of New Rochelle, the injected material in many places surpasses in quantity the amount of the original gneiss. Parallel injection is also finely illustrated by many of the districts of pre-Cambrian gneisses in eastern Canada, and western North America. In the latter region one of the most beautiful illustrations is that of the Madison Canyon gneiss in Montana. In these regions the intrusions seem to have occurred when the rocks were rather deep-seated, and doubtless in this zone intrusions are of much greater importance than nearer the surface. However, within a few thousand feet from the surface, extensive intrusions may take advantage of joints, faults, or radiating fractures. This is illustrated by the numerous granite dikes along joints in the Sierra Nevada granite; by the dikes of Crazy Mountain, Montana;[1] by the trap dikes of the Triassic of the Connecticut valley; by the dikes of Cape Ann, Massachusetts, where, according to Shaler,[2] the dikes occupy from 5 to 10 per cent. of the surface of the country; by the dikes of western Scotland;[3] and by the dikes of many other areas.

Besides the parallel and radiating intrusions just considered, great irregular intrusions of material have occurred on a vast scale. Irregular intrusions are especially numerous among the older rocks. The injected material may occupy a large part of the surfaces of the districts affected. The irregular injections of igneous material find lateral space largely by mashing or corrugating adjacent rocks, and this causes vertical expansion of the crust. Irregular intrusives may be found in the same districts in

[1] Livingston folio, by JOSEPH P. IDDINGS and WALTER H. WEED: Geol. Atlas of the United States, No. 1, 1894.

[2] The geology of Cape Ann, Massachusetts, by N. S. SHALER: 9th Ann. Rept. U. S. Geol. Survey, 1889, pp. 579–602.

[3] Geological map of Scotland, by SIR ARCHIBALD GEIKIE.

which the parallel injections are also found. Scores of illustrations of irregular intrusions, so extensive as to occupy important or even major parts of various districts, could be mentioned of any of the great groups of rocks. A number of instances are given on pages 49–50.

All openings which may be taken advantage of by injection may also be taken advantage of by underground water deposits, and thus by a combination of fracturing and cementation the area of the rocks is increased. That the rocks may thus receive an important extension of their surface area has been noted by Shaler.[1] While parallel and irregular cementation by water solution may not be so important as igneous injection in the lateral extension of rocks, it is a more widespread phenomenon, and undoubtedly has an important effect. Wherever openings have been produced in relatively deep-seated rocks (that is, in the lower part of the zone of fracture, and in the zone of combined fracture and flowage[2]) it appears to be the rule that cementation follows them, and thus rock material again occupies the entire space.

In regions where fissility has been developed, the laminæ are cemented by layers of infiltrated material, which in many places average as wide as the laminæ cemented. This is seen at many localities in the southern Appalachians. Cementation is not more important in closing the spaces between laminæ than it is in closing joints, faults, and irregular fractures. Such cementation may be found in the same districts as the depositions along the planes of fissility, and thus double the effect, or it may occur in districts in which fissility is unimportant. The Marquette district of Michigan finely illustrates the latter. Entire formations have been broken by innumerable joints, irregular cracks, or even brecciated. The openings are now entirely closed by cementation.[3] Since the time of this cementa-

[1] The crinetic hypothesis and mountain building, by N. S. SHALER: Science, Vol. XI, 1888, pp. 280–281.

[2] Principles of North American pre-Cambrian geology, by C. R. VAN HISE: 16th Ann. Rept. U. S. Geol. Survey, Pt. I, 1896, pp. 589–594, 601–603.

[3] The Marquette iron-bearing series of Michigan, by C. R. VAN HISE and W. S.

tion, when the rocks had neared the surface by denudation, other fractures formed which have not been cemented.

Thus throughout regions in which injection or cementation is extensive, there is complete evidence of important local extension of the crust of the earth. Moreover, in the case of the igneous material, it is certain that it acts as a wedge forcing the material apart. It also is possible that the wedging effect of cementation may not be unimportant. While it is probable that upon the average the deformations which produce the fractures taken advantage of by the entering material resulted in shortening the crust of the earth, it is by no means certain that in many cases at least the extension of cementation or injection did not largely compensate for the shortening due to the deformation.

Shortening of Algonkian and Archean rocks.— No one yet has been bold enough to attempt a quantitative estimate of the shortening represented by the older mountains, the stumps of which only remain. But oftentimes it has been stated in a general way that probably the pre-Cambrian folding, and consequent shortening, is as great or greater than all subsequent folding.[1] While I am not able to disprove this conjecture, it seems to me that the closeness of corrugation assumed as general for the ancient rock is not justified by the facts. I shall separately consider the Algonkian and Archean rocks because they are so dissimilar.

In many regions the Algonkian sediments are not closely plicated. For instance, in the Lake Superior region, including the Original Huronian district, the Keweenawan and Upper Huronian sediments are very gently folded. The same statement applies to other extensive areas of pre-Cambrian sediments in Canada. The sedimentary rocks of the Adirondacks are more severely folded, but still the folding is not close. The crystal-

BAYLEY: Mon. U. S. Geol. Survey, No. XXVIII, 1896, Pls. VII, VIII, IX, XXIII, and XXVI.

[1]A criticism on the contractional hypothesis, by C. E. DUTTON: Am. Journ. Sci., Vol. VIII, 1874, p. 121. Origin of mountain ranges, by T. MELLARD READE: London, 1886, pp. 133–153.

line and semicrystalline rocks of the Blue Ridge in some places are closely folded and have secondary structures, but are in many places not closely corrugated. For instance, the quartz-schists of Tullulah mountains are in very gentle folds. The folding of the pre-Cambrian sediments in western America is also rather simple. The thick Grand Canyon series is but gently undulating. The Uinta sandstone is in a great simple arch. The thick pre-Cambrian series of Montana is gently folded. The pre-Cambrian of the Wasatch and Medicine Bow mountains are somewhat more closely corrugated, but not nearly so closely as many areas of Paleozoics in the New England region. However, there are some districts in which the folding is close and complex. This is the case with the Lower Huronian of the Vermilion and Menominee districts, and to less extent of the Marquette district, all in the Lake Superior region. The folding of the Original Laurentian district is of the most complex kind. However, even in these districts of close folding, it cannot be stated that the shortening is greater than is the case in the closely folded Tertiary rocks of the Alps.

Thus it appears that somewhat gentle folding is the rule with the pre-Paleozoic sedimentary rocks, as with the Paleozoic and post-Paleozoic, but in occasional districts the deformation, as in the later rocks, is of the most intense character. Therefore, during early geological periods, as during later geological time, orogenic movements have been concentrated along definite zones. Apparently since the beginning of Algonkian time large parts of the continents have escaped violent orogenic movements.

From the foregoing I do not mean to assert that the pre-Paleozoic sedimentary rocks are upon the average not more closely folded than later rocks. Indeed, the reverse must be the case, for the earlier rocks have partaken in subsequent foldings. The point upon which I insist is that there is no such great difference in the amount of deformation as has been thought by many.

However, it is in the Archean rocks that the apparent plications are most severe, but it is to be remembered that we have here no

criterion upon which to make an accurate judgment, as bedding is missing. As seen (p. 29), cleavage is no criterion upon which to make estimates of shortening, and this is especially true of monoclinal cleavage, and such monoclinal cleavage is found in the Archean for great distances in various places, as for instance, in the Blue Ridge and Piedmont plateau, in southwest Montana, and in various areas in Canada. Also banding is no criterion, for, as has been seen in this paper, and shown in another place,[1] the regular banding in the Archean rocks is in many cases probably due to cementation and injection. However, it is often found in these ancient rocks that the secondary structures themselves, such as slatiness and schistosity, are folded into undulations, but these are in most cases rather gentle. For instance, the schistose structure of the Blue Ridge at Doe River is a single anticline, and on the Nacoochee-Hiwassee section are two anticlines separated by a syncline. The descriptions of Emmons and King show the same simplicity of structure for the Front Range of Colorado. The undulations of the schists are so gentle that they took them to be the remains of sediments, and gave an estimate of their thickness.

Finally, wherever we find exceedingly irregular and intricate structures in which no estimate can be made of the corrugations, even of the secondary structures, we are sure to find intrusive material intricately interposed, which may itself largely or wholly compensate for the shortening which we see.

I therefore conclude with the present state of knowledge, that we are wholly unable to make any quantitative estimate of the amount of crustal shortening involved in the deformation of the Archean rocks.

Longitudinal shortening of mountain systems.—In all past estimates which have been made of shortening in mountain-making only the transverse shortening has been considered, but in order to obtain a true estimate of the effects of deformation, it is necessary to consider the amount of longitudinal shortening. If it

[1] Principles of North American pre-Cambrian geology, by C. R. VAN HISE: 16th Ann. Rept., U. S. Geol. Survey, Pt. I, 1896, pp. 662–668, 684–688.

be agreed that complex deformation is the rule instead of the exception, as I have maintained in another place,[1] it is evident that longitudinal shortening is an important factor in deformation. As a consequence of cross folds, of reverse faults, and of other cross structures, it may be that the shortening of the crust of the earth in a longitudinal direction during the mountain-making processes is as great or greater than the transverse shortening.

This becomes evident as soon as the ratios between the length and breadth of the mountain chains are considered. The Appalachian system, in its broadest sense, extends from Alabama to the St. Lawrence River, a distance of about 1300 miles. Its breadth is about 75 to 100 miles. The ratio between the length and breadth is about 15 : 1. The Cordilleran system of North America in its broadest sense extends from western Alaska to southern Mexico, a distance of about 4800 miles. The breadth varies from 100 miles at the ends of the system to 1000 miles at the middle. But where this greater breadth occurs there are considerable distances between the different chains so that the folded area is probably not more than from one-half to one-fourth of the total amount. The average width of the folded part is probably somewhere between 300 and 500 miles. Thus the ratio between the length and breadth in this case would be between 16 : 1 and 10 : 1. The Andean system extends the entire length of South America, 4500 miles. This system is a comparatively narrow one, its average width being about 200 miles. The ratio of length to breadth in this case is therefore about 22 : 1.

It thus appears in the cases of these great mountain systems, if the longitudinal shortening involves from one-tenth to one-twentieth as much shortening as the transverse deformation per mile of linear distance, that the shortening of the crust of the earth as a result of the existence of the mountain systems is as great longitudinally as transversely.

In the case of some mountain systems which have considerable breadth as compared with their length, as for instance

[1] Loc. cit., p. 626.

the Pyrenees and the Himalayas, it appears that the longitudinal shortening is relatively more important than in the mountain systems in which the ratios between length and breadth are greater, as in the cases above mentioned. In the case of the Pyrenees this is beautifully brought out by the memoirs of Roussell, which show that cross folds are here important.[1]

In the introduction of the neglected element of longitudinal shortening into the problem of crustal shortening, in mountain-making, we have a factor which, contrary to those above considered, increases the total amount of crustal shortening. In order to properly estimate the effect of the formation of a mountain system upon the area of the surface of the earth, we must know its length and breadth now, as compared with the original length and breadth of the rocks making the mountains. The amount of crustal shortening is then known in surface area, the only proper unit in which comparison can be made, for shortening along one line is of little importance unless it extends over some finite distance transverse to that line. However, the introduction of this element of longitudinal shortening very greatly complicates the quantitative estimation of the amount of shortening of the earth, and such an estimation, in a direction transverse to the mountains, as has been shown, is a sufficiently difficult task if all the factors are taken into account which should be considered.

Although aside from the purpose of this paper, it may be remarked in passing that one of the difficulties which have appeared to confront geologists is not real. Geologists, assuming that all shortening is in a direction tranverse to the length of mountain systems, have been puzzled by the resultant conclusion that the shortening of the crust of the earth is so largely concentrated in a direction transverse to the meridians.[2] When

[1] Étudé stratigraphique des Pyrenees, by JOSEPH ROUSSELL: Bull. Carte Geol de la France, Tome VI, 1893–4, and especially accompanying Pls. I to V.

Étudé stratigraphique des massifs Mantagneux du Canigou et de L'Albere, by JOSEPH ROUSSELL: Bull. Carte Geol. de la France, Tome VIII, 1896–7.

[2] A criticism upon the contractional hypothesis, by C. E. DUTTON: Am. Journ. Sci., Vol. VIII, 1874, pp. 122–123.

one recognizes that longitudinal shortening may be as important as transverse shortening, this difficulty disappears.

Finally, to know the real effect of the deformation in mountain folding it is desirable to know not only the average closeness of the plications in two directions, but the depth to which this average closeness has been observed. In short, in order to obtain the most significant estimates of the effects of crustal shortening, not one dimension of a folded mountain mass, but three, length, breadth, and depth or thickness, should be taken in account. So far as I know, two of these factors, length and depth, have been wholly ignored in all estimates of shortening. The reason for this doubtless is that the difficulties in the way of the consideration of all these factors are insuperable, at least at present.

Shortening of removed formations.—Another element of uncertainty in giving estimates of crustal shortening is the unknown shortening of the rocks which have been removed by denudation. If, as has been supposed, circumferential shortening is at a maximum at the surface of the earth, the strata which have been most deformed have been removed from time to time. It would therefore follow, if we could estimate the amount of total shortening to which the rocks of the crust have been subjected, that this amount would fall short of the real shortening which the surface of the earth had undergone. The erosive forces as they cut off the mountain tops and distribute the material upon the border of the sea, smooth out the earth's wrinkles of age.

Conclusion.—If the argument of the foregoing pages holds, it is clear that we must begin at the beginning in making estimates of the amount of crustal shortening involved in mountain-making. The published estimates have ignored so many factors which must be considered before an estimate can have any quantitative value, that I am forced to the position that they are little more than guesses. It is therefore concluded that the amount of shortening of the crust of the earth, due to its deformation, is an entirely unknown quantity. By this I do not mean to imply that the crustal shortening has not been

very great, but that as yet we can only make qualitative statements in reference to its amount; that quantitative statements are objectionable because they imply a definiteness of knowledge not warranted by the facts, and therefore stay the progress of investigation.

PART II. CAUSES OF CRUSTAL SHORTENING.

Secular cooling.— The first cause of contraction to be considered, and the only one ordinarily considered, is secular cooling. The amount of such contraction has been variously estimated. But the largest calculated amounts which the physicists will allow have always been disappointingly small to the geologists.

Mallet, on the hypothesis that the earth was liquid and had a mean temperature of 4000° F., has concluded that the earth "between its period of liquidity and its present state has shrunk in diameter by 189 miles at the least."[1] At that time, according to Mallet, the earth would have a mean radius of 4053.3 instead of 3958.8 miles. The surfaces of these spheres would be respectively about 206,457,000 and 196,942,000 square miles, and thus the surficial contraction of the earth would be about 9,515,000 square miles.

Dutton, making his calculation on another basis, concludes if the earth once had a nearly uniform temperature of 7000° F., that "if we were to assign thirty miles as the diminution of the earth's mean radius since the first formation of a cooled exterior, we should probably reach the utmost limit consistent with Fourier's theorem."[2] Taking the average radius of the earth as 3958.8 as before, the radius of the earth before contraction, according to Dutton, could not be more than 3988.8. The surface of this expanded earth would be about 199,938,000 square miles, which gives a surficial contraction of about 2,996,000 square miles.

[1] Volcanic energy: an attempt to develop its true origin and cosmical relations, by ROBERT MALLET: Trans. Roy. Soc., Vol. CLXIII, 1873, p. 205.

[2] A criticism upon the contractional hypothesis, by C. E. DUTTON: Am. Jour. Sci,. Vol. VIII, 1874, p. 121.

Fisher calculates that the radial contraction of the globe has been .65 miles since its temperature was 4000° F., and 1.9 miles if its temperature were ever 7000° F. These calculated contractions are so slight that it is not worth while to calculate the surficial contraction which would result from them.[1] Certainly, if Fisher's conclusion is approximately correct, loss of heat by secular cooling is not even an important cause for crustal shortening.

Darwin,[2] as a result of a discussion of the strains of the crust resulting from secular cooling, concludes that an earth 8000 miles in diameter would contract so that "in 10,000,000 years, 228,000 square miles of rock would be crumpled up and piled on top of the subjacent rocks."

The variation in the estimates above given is so great that the question not unnaturally arises as to whether the truth may not be far from any of them. Indeed Darwin says, with reference to his estimate, that "the numerical data with which we have to deal are all of them subject to wide limits of uncertainty."

All of the foregoing calculations as to the amount of heat lost by the earth are based upon the hypothesis that the earth has not had a higher average temperature than 7000° F. during geological time, and also on the hypothesis that the entire loss of heat is by conduction. If the present temperatures deep within the earth are to be measured by *many* thousands of degrees, as some believe, the amount of heat lost would be much greater than calculated, and the resultant contraction correspondingly important. Also the process of cooling would have been much more rapid if convectional currents assisted, by means of which the hotter material comparatively deep within the earth continued for a long time to be brought near or to the surface. It has been customary to consider the heat lost through convection as so small as to be negligible, and all calculations upon the amount of heat lost by secular cooling have ignored this quantity.

[1] Physics of the earth's crust, by OSMOND FISHER: London, 1881, p. 72.

[2] Note on C. Davison's paper on the straining of the earth's crust in cooling, by G. H. DARWIN: Phil. Trans. Roy. Soc., Vol. CLXXVIII, Pt. A, 1887, p. 249.

Loss of heat by convection is accomplished through transfers of magma and transfers of water.

If calculations are made of the loss of heat due to vulcanism, upon the basis of the volcanic materials brought to the surface of the earth at the present time, it is highly probable that the conclusion would be reached that this quantity is so small that it might be ignored. But one must remember that present vulcanism is no criterion by which to estimate the transfers of material which have resulted during past great periods of regional vulcanism. The transfers of enormous quantities of igneous material by vulcanism from deep within the earth to its outer shell or to the surface of the earth, described on page 48, strongly suggests that convectional currents have been a more important factor in the process of secular cooling that has been supposed. A large part of the heat which is carried toward the surface of the earth by magmatic convection is transferred only a part of the way by the liquid rock. For, as seen (p. 49), the intrusive rocks probably equal or exceed in quantity the extrusives, and deeper transfers may have occurred of which we have no definite knowledge. From the place where the magma is stayed the heat is brought to the surface in two ways. First, a part is transferred by conduction through the overlying mantle of rock. That such conduction occurs is shown by the fact that the temperature gradients in many districts receiving hotter material are higher than the average. Second, another part of the heat is brought to the surface by convection through underground waters. In this case the transfer of heat begun by the magma and by conduction is continued by water.

This brings us to the second agent by means of which the earth is losing heat through convectional currents. Underground water circulation everywhere pervades the outer zone of fracture at the present time, and doubtless has since a solid crust existed. That heat is brought to the surface of the earth by water is self-evident in the various districts of geysers and hot springs.

But in estimating the amount of heat which escapes by convectional transfer by underground waters, it is not sufficient to con-

sider the random hot springs. If these were the basis of calculation it would indeed be unimportant. But if underground waters upon the average reach the surface with a slightly higher temperature than when they entered it, this may be a very important means by which heat is lost through convection. While difficult or impossible to prove by observation, I think it is unquestionable that underground waters must escape at a temperature upon the average somewhat higher than that with which they entered the earth. The average temperature of water when it enters the land may be presumably taken as that of the average of the surface of the earth at that locality. From this average temperature at the surface the temperature of the rocks increases downward. The vast quantities of water which at all times is taking an underground journey gains heat as a result of its contact with the warmer rocks. At another time I shall attempt to show that the water thus heated finally reaches the surface without losing all of the heat gained in its downward course. If this be so, there is constant loss of heat.

So far as I know, no attempt has ever been made to estimate the heat lost to the earth by means of the convectional underground currents of magma and water. While I am wholly unable to prove it, I have no doubt that the absolute quantity of such heat is enormous. The heat transferred by convection is to be added to the amount transferred by conduction.

As having an effect opposite to that of conduction and convection in lowering the average temperature of the earth, it is to be noted that as a result of changing rotation period (considered pp. 54–59) heat develops within the earth in two ways. First, heat is developed by tidal friction. Darwin states that he has "calculated that the heat generated in the interior of the earth in the course of the lengthening of the day from 5 hours 36 minutes to 23 hours 56 minutes would be sufficient, if applied all at once, to heat the whole earth's mass about 3000° F., supposing the earth to have the specific heat of iron."[1] Second,

[1] On the precession of a viscous spheroid, and on the remote history of the earth, by G. H. DARWIN: Phil. Trans. Roy. Soc., Vol. CLXX, Pt. II, 1879, p. 535.

consequent upon self-condensation, as a result of increased pressures coming from the greater effectiveness of gravity as the rotation period increased, additional heat within the earth would be developed. Also condensation of the earth as a result of change of physical state (see pp. 59–61) or in any other way, would result in the development of heat.

What the residual effect of these opposite neglected factors is upon the loss of heat as ordinarily calculated, it is impossible to say, and until the various estimates of the loss of heat approach one another more nearly than they do at present, it is not worth while to make a conjecture upon this subject.

If it be true that the temperature of the interior of the earth is much higher than premised in the calculations of heat lost during secular cooling, and if the convectional movements of magma and water are important means of refrigeration, it may be that heat has been transferred to the surface from much deeper within the earth than estimated by the physicists. Dutton[1] states that below "200 or 300 miles the cooling has, up to the present time, been extremely little." Davison affirms that below 400 miles the earth has not sensibly cooled.[2] These figures are based upon the hypothesis that the loss of heat is due wholly to conduction in a globe having a uniform initial temperature of 7000° F. If this hypothesis is incorrect, the hypothetical level of no lateral stress would be at a greater depth than calculated by Reade, Darwin, and Davison,—from 2 to 8 miles.[3]

Whatever the total loss of heat as a consequence of the various positive and negative factors, if we assume a liquid earth, it is certain that all of the resultant contraction is not available

[1] A criticism upon the contractional hypothesis, by C. E. DUTTON: Am. Journ. Sci., Vol. VIII, 1874, p. 120.

[2] On the distribution of strain in the earth's crust resulting from secular cooling; with special reference to the growth of continents and the formation of mountain chains, by CHARLES DAVISON: Phil. Trans. Roy. Soc., Vol. CLXXVIII, Pt. A, 1887, p. 235.

[3] Estimates summarized in Manual of geology, by JAMES D. DANA: 4th edition, 1895, pp. 384–385.

for crustal corrugations. None of the contraction is available to explain surficial deformation until after a solid outer shell has formed, either by solidification from the center or from the surface.

Also, making no hypothesis as to the early condition of the earth, the lessening of the solid surface available for corrugation does not include the full amount obtained by calculations based upon radial contraction. So far as the exterior shell was hotter than at present, its cooling would cause circumferential contraction, and consequent lessening diameter, without crustal corrugation, just as in the case of a steel jacket which in a heated condition is put upon the core of a gun, and which upon cooling shrinks.

Furthermore, as pointed out by Davison, the outer spherical shell might continue to contract circumferentially faster than the average contraction of the interior, because nearer the surface, and more rapidly losing heat.[1] This would cause tension in the outer part of the earth, just as in the case of the jacket on the gun, which, after it has shrunk to the core, continues to contract and so firmly clasps the core as to be under great tension; or just as a large steel ingot, at a high uniform temperature, by rapid cooling may so much more rapidly contract on its outer part than in its core as to form surface tensional cracks, because of the tensile stretching during the early stages of cooling.

How important this circumferential contraction is in the case of the earth is unknown. For we do not know the average temperature of the outer shell of the earth during early geological time, nor do we know very exactly its present temperature. We can only say that certainly some quantity must be subtracted from the total surficial decrease, resulting from loss of heat, in order to obtain the amount which is available to account for crustal shortening. Estimates which disregard this correction would be true only so far back in geological time as we can assume the temperature of the outer shell to be practically the same as at present.

[1] Loc. cit., pp. 231–242.

Also (as explained p. 53), so far as heat is lost by means of vulcanism the resultant earth contraction does not give an effect in crustal corrugations in addition to that due to the transfer of the magma.

But in estimating the effect of secular cooling upon corrugation, it must be borne in mind that the earth is so large that corrugations may have begun on one part of the crust, while other parts were still subject to tension, as explained, page 46. Under these circumstances corrugations might be produced which would be compensated by tensile cracks elsewhere. Such tensile cracks may become filled with sediment, with vein material, or with igneous injections. In so far as such compensated corrugations have been produced during the early history of the earth, these deformations are in excess of the amount which it is allowable to attribute to secular cooling. Davison suggests that early in the history of the earth the continental masses might have passed earlier from the stage of tension to the stage of compression than the sea beds, and that a part of the crustal flexures of the continents were, therefore, compensated by tensile fractures under the sea.[1]

It appears from the foregoing that the corrugations of the earth due to secular cooling follow from the difference in the loss of heat by conduction and by convection, and that developed by earth movements; and from an irregular distribution of the resultant stresses, which in some places may be tensile, and at the same time in other places be compressive. Ordinarily the loss of heat by conduction only has been considered. It is clear that in secular cooling we have an important, but probably by no means an adequate, cause to account for observed crustal deformation.

Vulcanism.—The second cause to which I shall appeal to explain shell corrugations is vulcanism.

At the outset it should be said that the quantity of igneous material which now reaches the surface of the earth is no criterion by which to judge past extrusions, for at times of regional

[1] Loc. cit., p. 241.

extrusions a quantity of magma may be emitted which surpasses the entire amount emitted between epochs of regional extrusions.

To appreciate the importance of regional extrusions of magma I need only to recall the Tertiary volcanic period, during which were produced the great lava plateaus, some of them thousands of feet in thickness, in western North America, Great Britain, Iceland, Franz Josef land, New Zealand, Abyssinia, and India. In western North America the area of these volcanics is to be estimated by hundreds of thousands of square miles. The Deccan traps of India are estimated to cover 200,000 square miles and for much of this area to be from 2000 feet to 6000 feet thick.[1] But the Tertiary volcanics with which we are acquainted are only a remnant of the quantity emitted; for during Tertiary and post-Tertiary times, the erosion has been stupendous, and a large fraction of the material extruded has been converted into sedimentary rocks by means of the epigene agents.

While the volcanic rocks of the Tertiary period surpass in quantity the known igneous rocks of any previous period, it by no means follows that previous volcanic extrusions might not have been on a still vaster scale. For the further back we go, the larger is the fraction of the volcanic rocks of any given period which has been converted into sedimentary rocks by the epigene agents, and, furthermore, the proportion of the volcanics of a given period which is buried under sedimentary and igneous rocks ever increases as time passes by, so that but a small fraction of the formations bearing extrusives of great age is exposed, and in these formations, as has been seen, the larger parts of the extrusives have been destroyed.

Moreover, the extrusives are probably but the smaller part of igneous rocks. In another place I have suggested reasons why intrusives are more extensive than extrusives.[2] For the

[1] Geology of India, by R. D. OLDHAM: 2d ed., Calcutta, 1893, pp. 256, 263.

[2] Earth movements, by C. R. VAN HISE: Proc. Wis. Acad. Sci., Arts, and Letters, Vol. XI, 1898, pp. 495–496.

present purposes it is not necessary to enter into this discussion, but I wish to recall the facts as to the dominance of intrusives. Intrusive rocks are discoverable only after a region has been eroded, and it is therefore in these denuded regions that we are to look for evidence of intrusion. Beginning with the older periods, and confining our attention to America, we find that the Archean, so far as we can ascertain its original character, consists largely of modified plutonic rocks. Passing to the Algonkian, hardly an area is found in which intrusive rocks do not occupy a large percentage of the area. This is illustrated by the great masses of intrusive rocks in the Lake Superior region, which in many districts occupy large fractions of the areas. In the Rocky mountain region, in various districts, the Algonkian sedimentary rocks are subordinate to the simply enormous quantities of intrusive granite and other rocks. This is well illustrated by the Pikes Peak district, where, according to Cross,[1] the intrusive granite occupies two-thirds or three-fourths of the entire area of the one-half-square-degree quadrangle, and where the Algonkian sediments are mere fragments; by the Black Hills; by the Medicine Bow mountains, and many other ranges. Passing to the Paleozoic and Mesozoic, in almost every mountain region there are enormous masses of intrusives. This may be illustrated by the great batholiths[2] of granite in the Sierra Nevada and in the New England regions, by the laccoliths of the Henry mountains, of the Elk mountains and La Plata mountains, and by irregular intrusions, sills, and dikes, in almost every mountain district in the country. As yet the known Tertiary intrusives in America are not so important, but in Great Britain, where denudation has gone far, a vast quantity of the Tertiary intrusives has appeared. Doubtless in America also, when denudation shall have advanced far enough, correlative with the volcanics mentioned p. 48 will be found a great quantity of intrusive rocks.

[1] Pikes Peak folio, by WHITMAN CROSS: Geol. Atlas of the United States, No. 7, 1894.

[2] Suess's term *batholith* is here used in its strict etymological sense, with no reference to any theory as to how the magma was transferred, or as to whether or not it occupied previously existing spaces.

What is true of the intrusive rocks of America is true of other regions of the globe. I have selected America as an illustrative continent, because I know the facts of the field better there than elsewhere.

Any terms which one can use must fail to convey an adequate idea of the stupendous quantities of magma which have been introduced into the outer shell of the earth, or poured out upon its surface. It is clearly impossible to make even an approximate quantitative guess of the amount of igneous materials which have thus been intruded and extruded during geological times. Its quantity is certainly to be measured in tens of millions of cubic miles, rather than in smaller units.

Now in this transfer of earth material two things have happened. In so far as it has been taken from the nucleus, it has lessened its bulk. By the amount the nucleus has been lessened, the bulk of the shell has been increased.

Of this great mass which has thus migrated from the nucleus to the shell, a large proportion has stopped before reaching the surface. This is only possible by extension of the shell either vertically or laterally, or both. The forms of intrusives clearly show that both have locally occurred. Sills and laccoliths have mainly found a place to occupy by vertical extension of the shell, although to some extent lateral extension of the intruded layers (see p. 15) is also produced by them. It is equally clear that volcanic necks, dikes, and batholiths have largely found space by local lateral extension, although it is not doubted that the intrusion of such forms is also accompanied by vertical extension, and in the case of batholiths an important amount. Necks, dikes, and batholiths have formed in cracks and crevices, and wedged the walls apart, thus locally extending the crust, and giving surface which may be used in lateral mashing or corrugations elsewhere. In many cases the mashing and corrugation, and consequent thickening and vertical extension, are immediately adjacent to the intrusives. This is most marked in the case of the great batholiths. Adjacent to such enormous masses as the batholiths which are found in the Black Hills, in

the Lake Superior region, in western Massachusetts, and in Great Britain, slaty or schistose structures parallel to the intrusives are common. These structures are conclusive evidence of lateral compression and vertical extension of the rocks intruded.

As already noted, the whole of the enormous mass of the intrusives and extrusives is to be subtracted from the mass of the nucleus and added to the mass of the shell. Of these two effects the expansion of the crust is without doubt by far the more important. If the nucleus of the earth be taken as having a radius of 3900 or more miles, radial contraction of one mile would involve a loss of volume of more than 190,000,000 cubic miles. A contraction of the radius of the earth of one mile, that is, from 3958.8 to 3957.8, would give a surficial lessening of only 100,000 square miles. In the supposed case of nucleal contraction of the radius by one mile, the 190,000,000 cubic miles of material would be available for additions to the crust. If it be supposed through geological time that this amount of material has been uniformly intruded within the outer ten miles of the crust of the earth, this would demand a surface space of 19,000,000 square miles, or about one-tenth of the earth's surface. As a consequence the material previously occupying this outer shell would be crushed so as to occupy nine-tenths of its original space, and this would involve enormous lateral crustal corrugation, with consequent thickening of the outer shell from ten miles to about eleven miles.

If it be supposed that the transfer through geological time from the nucleus to the outer five miles of the crust has been only one-tenth of the amount suggested in the above paragraph, the effect would still be great. Under this supposition, the radius of the nucleus of the earth as a result of igneous intrusions has contracted one-tenth of a mile, and as a consequence its surface has been lessened by about 10,000 square miles. This would involve an intrusion into the outer five miles of the crust of the earth of about 19,000,000 cubic miles of material, and I suspect that this is an underestimate rather than an overestimate of the igneous intrusions in this outer shell of the earth. Suppos-

ing that the igneous material is uniformly distributed vertically through the outer five miles, the material would occupy a surface space of about 4,800,000 square miles, with consequent surficial contraction and thickening of the remaining material of the crust. The surficial shortening of the original crust involved would in this case be about one-half as great as that due to secular cooling throughout geological time, as calculated by Mallet, and more than one and one-half times as great as that calculated by Dutton (see p. 41), even if it were supposed that the entire contraction were available for crustal corrugation.

Of course the above figures are hypothetical. The purpose of introducing them is to show the relative importance of crustal corrugation as a result of intrusion and nucleal contraction due to the transfer of magma, and to emphasize the fact that vulcanism is probably one of the great causes for shell corrugations, for two reasons. The intrusives occupy space in the shell. The nucleus shrinks by an amount equal to the combined igneous intrusions and extrusions. I am inclined to believe that this cause for crustal deformation is of the same order of magnitude as that due to secular cooling.

The fact that periods of considerable orogenic movements generally correspond with periods of great vulcanism is very suggestive and supports the conclusion as to the importance of the above transfers of igneous material, in explaining crustal corrugations. As a single illustration of this principle of correspondence may be mentioned the fact that the great Tertiary mountain-making period in which the Sierra Nevada range was last uplifted, in which the Coast Ranges and St. Elias Alps were formed, in which the Alps themselves were produced, and in which other mountain ranges were formed, is contemporaneous with the great Tertiary period of vulcanism.

By the foregoing I do not mean to imply that vulcanism is the initial cause of the orogenic movements. The initial causes are those assigned for earth contraction. The transfers of material followed as a result of the action of the initial causes, and thus is in a measure an effect, but also where the transfer

occurs this is a further cause for crustal corrugation. Thus the transfers of magma are both effect and cause of crustal corrugations.

So far as I know, Lyell[1] was the first to suggest that there is a connection between folding and igneous intrusions. However, Fisher[2] went further than Lyell, and urged that vulcanism is the chief cause of crustal corrugation. His argument may be very briefly summarized as follows: Fissures form "through metamorphic changes. When these fissures originated below and are propagated upward, they become filled with elastic vapor, and compression results." According to Fisher, it is the expansive force of the vapor which makes the openings, and consequent corrugations, and these openings are occupied by the magma. So far as my present purposes are concerned it makes no difference how the intrusives found places for themselves. I merely insist upon the fact that somehow great spaces formerly occupied by solid rocks came to be occupied by the magma.

Shaler[3] has also appealed to igneous intrusions as a cause for mountain-making, and in a manner similar to Fisher. He thinks that in many places of New England the dikes occupy from one-twentieth to one-tenth of the superficial area.

However, neither Fisher nor Shaler consider the shrinkage of the nucleus of the earth due to the loss of the magma for both intruded and extruded materials, or the crustal corrugation which must result from this transfer of material.

In closing this part of the subject, it should be noted that crustal corrugation caused by transfers of magma involves no contraction of the earth nor lessening of its surface as a whole, except as magmatic transfer results in loss of heat by convection, as explained (p. 43). It may also be remarked that the earth contraction due to loss of heat caused by actual transfers of

[1] Principles of geology, by CHARLES LYELL: 10th ed., London, 1867, Vol. I, pp. 134–135.

[2] Physics of the earth's crust, by OSMOND FISHER: London, 1881, pp. 185–207, and pp. 284–286.

[3] The crinetic hypothesis and mountain-building, by N. S. SHALER: Science, Vol. XI, 1888, pp. 280–281.

magma to within the crust or upon its surface is not a cause for crustal corrugation in addition to that produced by the transfer itself.

Cementation.—Another cause which explains crustal corrugation is cementation (see pp. 34–35). In this process material is carried in a direction opposite to the transfers of vulcanism. In the outer zone of disintegration and decomposition material is everywhere taken into solution by underground waters, and carried to the openings below, where a part of it is deposited. Although the zone of solution which supplies the material at any time is narrow, material never fails, because this outer zone is ever migrating downward. Wherever at moderate depth during the process of deformation openings form, unless they are occupied by magma, they are gradually filled by water deposits, and thus there is local lateral extension, as in the case of vulcanism. The amount of material which thus migrates downward by means of underground waters cannot be quantitatively estimated, but it is certain that it is enormous.[1] In many regions where much deformed, comparatively deep-seated rocks have been brought to the surface, it is found that a measurable, and in some cases a considerable percentage of the entire space was once unoccupied and has been filled by cementation. The cemented rocks thus become a unit, which may be later deformed themselves, or transmit the thrusts to adjacent rocks, which may be deformed. In either case the shortening of the original material is compensated, at least in part, by the extension due to the cement, and thus the crustal corrugations are partly explained by water transfers of material.

Change of oblateness.— Peirce[2] and Darwin[3] have shown that as a result of tidal retardation the speed of rotation of the earth is decreasing, and that in the far distant past it rotated much more

[1] Earth movements, by C. R. VAN HISE: Proc. Wis. Acad. Sci., Arts, and Letters, Vol. XI, 1898, pp. 511–512.

[2] The contraction of the earth, by B. PEIRCE: Proc. Am. Acad. Arts and Sci., Vol. VIII, 1873, pp. 106–108: Reprinted in Nature, Vol. III, 1871, p. 315.

[3] On the precession of a viscous spheroid, and on the remote history of the earth, by G. H. DARWIN: Phil. Trans. Roy. Soc., Vol. CLXX, Pt. II, 1879, p. 535.

rapidly than at present, at one time possibly as fast as once in five and one-half hours. During this time of changing rotation, assuming that the geoid has accommodated itself to its period of rotation in the past as at present, Peirce states that there was a "diminution of oblateness arising from the diminished velocity of rotation upon the axis." He concludes on the hypothesis of homogeneity, when the earth rotated 4.236 times as fast as at present, that the equatorial radius would have been about 2½ per cent. greater than at present.

Taylor[1] later calculated that "when the day measured but six of our hours, the equatorial radius (assuming a true ellipsoid of revolution, and neglecting the small amount of contraction by loss of heat) would have been about one-tenth greater than it now is, or 4359 miles, and polar radius about one-sixth less, or 3291 miles. In other words, the poles would have been about 658 miles nearer the center of the earth than they are at present, and the equatorial protuberance about 396 miles higher than at present."

The discrepancy between these two results is so great that I referred the problem, for re-solution, to Professor C. S. Slichter, whose paper on this and other points immediately follows (pp. 65–78). I further asked that he obtain the amount of surficial contraction which would result from the change of oblateness. Upon the hypothesis of homogeneity, and with a period of rotation of five and one-half hours, he obtains a result which is practically the same as that of Peirce's. He finds that the earth, instead of having a mean radius of about 3959 miles, would have a polar radius of about 3736 miles, and an equatorial radius of about 4076 miles. This change from the past oblate spheroid to the present oblate spheroid would involve a contraction of the surface of the earth of about 210,000 square miles.

Change of pressure.—It further occurred to me that when the earth rotated more rapidly, the centrifugal force was greater than at present. When the rotation was four time as rapid as at

[1] On the crumpling of the earth's crust, by W. B. TAYLOR: Am. Jour. Sci., Vol. XXX, 1895, p. 257.

present the centrifugal force at the equator would be sixteen times greater than now. This being the case, it is evident that the effectiveness of gravity in producing interior pressures in the earth must have been less than at present. If the pressures were less, other things being equal, the earth would have less density than at present, and thus by a steady increase in the effectiveness of gravity during the time of decreasing rotation, we have a cause for contraction of the earth.

After reaching this qualitative conclusion, I asked Professor Slichter to handle the problem quantitatively. He finds when the period was five and one-half hours, on the hypothesis of homogeneity, that the pressure at the center of the earth was 1,688,000 atmospheres, instead of 1,772,000 atmospheres, or 4.8 per cent. less. Following Laplace's hypothesis that the earth is heterogeneous, and increases from a density of 2.7 at the surface to 10.74 at the center, and supposing that the heterogeneous oblate spheroid had an eccentricity of .4, the same as the homogeneous spheroid which has a five and one-half hour period, he finds that the pressures at the center would be 2,920,000, instead of about 3,000,000 atmospheres, or 2½ per cent. less than now. Further, as suggested by Professor Slichter, if it be supposed that during the geological history of the earth there has been a steady change from homogeneity in the direction of heterogeneity, the pressures at the centers of the spheroid, instead of increasing by the small amounts given, might have increased a much larger amount, depending upon the amount of differentiation (see Fig. 2, p. 72). The extreme case would be a change of pressure from those at the center of the homogeneous oblate spheroid when the period was five and one-half hours, that is, 1,688,000 atmospheres, to the present pressures of the heterogeneous spheroid, 3,000,000 atmospheres. In this case the pressures would have been 43.7 per cent. less than at present. It is not supposed that any such change of pressure has occurred during geological time, but the truth probably lies somewhere between this amount and the minimum, 2½ per cent., and probably much nearer the latter amount than the former.

Calculating on the basis of a heterogeneous spheroid at the beginning, *i. e.*, upon the minimum change of pressures of $2\frac{1}{2}$ per cent., and assuming Laplace's laws of the relations of pressures and densities, that "The variation in pressure in the interior of the earth is proportional to the variation in the square of the density" (see p. 75), Professor Slichter finds the surface would be two-thirds of 1 per cent. greater than at present, or 1,700,000 square miles larger.

Moreover, when the surface and volume were greater than the present amounts, the effectiveness of gravity in producing pressures would be less than assumed, because of the greater size of the spheroid, so that the estimated enlargement of the surface is short of the truth. However, it does not appear practicable to make a quantitative estimate of the value of this element.

Another estimate of the amount of surficial lessening as a result of increased pressure may be made by a different line of reasoning, as follows: The most probable conjecture which can be made as to the average density which the material of the earth would have if it could all be placed under conditions of ordinary pressure and temperature is that obtained by Farrington as the average specific gravity of meteoric falls, 3.69.[1] The material of the crust probably does not represent the average composition of the earth, for differentiation must have occurred to some extent, upon any hypothesis as to the origin of the earth. All inferences as to the composition of the interior of the earth are based upon a considerable number of hypotheses, none of which are verifiable. However, the meteoric falls, not the finds, give us the density of the material which is now being added to the earth. This is probably a better guide as to the average composition of the earth than the average of meteoric finds, as suggested to me by Professor Chamberlin, because the stony falls of the past have probably largely disintegrated. Of course it is not certain that the material added at present to the

[1] The average specific gravity of meteorites, by O. C. FARRINGTON: JOURN. GEOL., Vol. V, 1897, pp. 126–130.

earth from the interplanetary spaces represents the average composition of that out of which the earth segregated, but I can see no prospect that we shall be able to make any better conjecture of the average composition than that based upon meteoric falls. As already noted, the average specific gravity of such falls is 3.69, and the specific gravity of the earth is 5.67. Now, if this increased density is due to pressure, notwithstanding the high average temperature of the interior of the earth, it follows that the volume of the earth, as a result of pressure, has been reduced in the proportion of 5.67 to 3.69. The former number is 53.65 per cent. greater than the latter. If it be supposed that this percentage of expansion in volume would be inversely as the pressure at the center, a decrease of pressure at the center of 2½ per cent. would represent an increase of volume of 1.34 per cent., and an increase of superficial area of about 1,650,000 square miles. It will be noted that the above numbers are so manipulated as to give a minimum result. They could be handled in a different way and give a larger contraction of the surface.

The correspondence of this result with that obtained by Professor Slichter, 1,700,000, by an entirely independent line of calculation, is notable and suggests that Laplace's hypothesis as to the relations of pressure and densities within the earth, and the hypothesis that the average specific gravity of the earth material at ordinary pressures and temperatures is 3.69, and that the present density of the earth, 5.67, is due to pressure, may possibly both be approximately true.

The above calculations are based upon the hypothesis that the matter of the earth remains in the same condition under all pressures. It is subsequently seen that by a change from a liquid to a solid crystalline condition there is an important contraction. The increased pressure due to lessening rotation may have carried this change further than it would otherwise have gone. Gilbert has suggested to me that a moderate change of pressure within the earth may have acted similarly to the pressure upon a spring. Until the pressure reaches a certain amount but little deformation occurs, but at a certain stage a little

added pressure produces important deformation. In another place (pp. 8–9) in this number of the JOURNAL, Powell suggests that the modulus of compressibility varies under different conditions, and that so slight a change of pressure as that due to unloading and loading by denudation, has caused important expansion and compression. If this be so, so important a change of pressure as results from change of the rotation period of the earth might have produced a more important effect upon its volume than would be obtained by supposing the modulus of compression to remain the same under all pressures.

It is not supposed that the numerical results given (pp. 56–58) for surficial lessening of the earth due to increased pressure, following upon lessened speed of rotation, approach exactness. However, it is to be noted that the numbers obtained by two different methods are concordant, and moreover, that all of the hypotheses used in obtaining these numbers have been so made as to obtain minimum results rather than maximum, and they are therefore probably much too small. It therefore appears highly probable that crustal shortening resulting from increased pressure as the speed of rotation of the earth has lessened, is one of the chief causes for earth contraction.

Change in physical condition.—Another cause of the earth's contraction is the change in the physical condition of the matter of the earth's interior. In so far as liquid material has changed to a solid amorphous material, this has produced contraction. Further, if liquid or solid amorphous material has changed to a crystalline condition, this has resulted in more important contraction.[1] This contraction is supposed to be due to the closer arrangement of the molecules. According to Delesse,[2] in passing from the crystalline to glassy state, granite decreases in density 9 to 11 per cent., syenite 8 to 9 per cent., diorite 6 to 8 per cent., dolerite 5 to 7 per cent., and trachyte 3 to 5 per cent. Barus has shown in the case of diabase, an

[1] So far as I am aware, Lyell was the first to suggest that deformation might result from a change from a liquid to a crystalline condition. (Principles of geology, by CHARLES LYELL: 10th ed., London, 1867, Vol I, pp. 134–135; Vol. II, p. 236.)

[2] See Manual of geology, by JAMES D. DANA: 4th ed., 1895, p. 265.

average rock, that it expands 13 per cent.[1] in changing from the crystalline to a liquid condition. The reverse passage from the liquid to the crystalline condition would involve a contraction of more than 12 per cent.

Even if the earth is now solid and crystalline to the center, as believed by some geologists, it by no means follows that this was the case through the major part of geological history. If the changes above mentioned have largely occurred during geological time, this has been a very important cause for contraction. However, there is no way by which the amount can be quantitatively estimated without involving so many uncertain hypotheses that it is not considered advisable to make the attempt.

Another subordinate cause for contraction is a change from less complex to more complex molecules. In so far as this change is involved in that of change from a liquid to an amorphous state or from either of these states to the crystalline condition, it has already been counted; but as a result of chemical interactions all substances, even crystalline compounds, tend to rearrange themselves under given conditions, especially where the temperature and pressure are great, so that they will have the most compact molecules. In so far as this has occurred, it is a cause for contraction, although its importance cannot be assumed to be great.

These changes in the physical state of matter and the consequent earth contraction are independent of the numerical results due to change of pressure and loss of heat given on a previous page; for all the estimates in reference to secular cooling and changing pressure are upon the hypothesis that the matter continues in the same state. The loss of heat and the increase of pressure are undoubtedly among the causes which promote change of physical condition, but in so far as change of state has occurred the resultant contraction must be added to the quantities assigned to the amounts due to secular cooling and increased pressure.

[1] The contraction of molten rock, by C. BARUS: Am. Jour. Sci., Vol. XLII, 1891, pp. 498-499.

Loss of water and gas.—Finally, as suggested by Fisher,[1] nucleal contraction may have resulted from loss of originally occluded water. Chamberlin suggests that water and gas may have been emitted which have been lost to the earth.[2] Both of these losses would result in contraction of the nucleus. Probably the quantitative value of such contraction and consequent crustal shortening is small.

General.—Doubtless as the study of the earth continues, causes other than those assigned will be discovered for crustal shortening.

However, it is believed that the cumulative effects of the various causes assigned for nucleal contraction, and for crustal corrugation, are possibly sufficient to account for the phenomena of mountain-making.

We have seen that there are four important causes for crustal corrugation. These are secular cooling, vulcanism, change of oblateness, and change of pressures. Possibly there should be included among the important causes also that of change in physical condition and cementation.

It is impossible to make any accurate quantitative comparison of the several causes. However, it is to be noted that the change in surficial area due to oblateness of 210,000 square miles is about equal to that which Darwin estimated would result from secular cooling in 10,000,000 years, 228,000 square miles. It is to be further noted that the contraction due to increased pressures at a minimum estimation, 1,700,000 square miles, is 7½ times as great as the amount which Darwin estimated would occur in 10,000,000 years as the result of secular cooling, and is therefore equivalent to the effects of secular cooling for 75,000,000 years, or for a longer period than Darwin allows for the history of the earth since the separation of the earth-moon couple. At present we are, and probably we shall long continue to be, unable to give any accurate quantitative value to the crustal shortening

[1] Physics of the earth's crust, by OSMOND FISHER: London, 1861, pp. 87, 180, 218.

[2] A group of hypotheses bearing on climatic changes, by T. C. CHAMBERLIN: JOURN. GEOL., Vol. V, 1897, pp. 656–668.

resulting from vulcanism and cementation and from change in physical condition, but it appears possible, perhaps probable (see pp. 47–52), that in vulcanism we have an explanation of as large, or even a larger, fraction of the phenomena of crustal corrugations than is furnished by any other single cause.

The various causes for crustal shortening may be divided into two classes: (1) those which involve a change in the volume of the earth; and, (2) those which involve transfers of material. (1) The loss of heat due to secular cooling, the increased pressures due to lessening rotation, and the changes of physical condition involve a contractional change of volume. Changing oblateness due to changing rotation, vulcanism, cementation and nucleal loss of water and gas, involve no appreciable change of volume. (2) Changing oblateness is only possible by deep-seated transfers of material which cause a change in the form of the earth resulting in surficial contraction. Vulcanism results in crustal expansion and nucleal contraction, and therefore in crustal corrugation. The surficial expansion due to cementation compensates for a part of the crustal corrugation. Loss of water and gas produces slight nucleal contraction, and consequently some crustal corrugation.

Furthermore, it is to be remembered that the entire effect of all these changes is available to account for crustal corrugation, with the exception of contraction due to loss of heat, which, as explained (pp. 44–45), is only partially available to account for crustal deformation. Upon the other hand, the transfers of material by vulcanism from the nucleus to within the shell has an added effect in producing crustal corrugation much greater than that due to nucleal contraction.

The crustal shortening due to changing oblateness, and increased pressures resulting from lessening rotation must have been large in the remote past. According to Darwin,[1] 56,810,000 years ago the rotation period of the earth was 6 hours 45 minutes, and 46,300,000 years ago the period was 15 hours 30

[1] On the precession of a viscous spheroid and on the remote history of the earth, by G. H. DARWIN: Phil. Trans. Roy. Soc., Vol. CLXX, Pt. 2, 1897, p. 494.

minutes, *i. e.*, in about 10,500,000 years the period changed 8 hours and 45 minutes. For the entire 46,300,000 years which have since elapsed the change in period was from 15 hours 30 minutes to 24 hours or a change of 9 hours 30 minutes, but a little more than the change for the previous 10,500,000 years. At the present time, changing rotation has ceased to be a cause for mountain-making of any importance, for, according to Cayley,[1] the acceleration of the moon's motion due to tidal friction is less than 6 seconds per century.

The chief effects in mountain-making of changing oblateness and increased pressures resulting from change of rotation, as noted by Peirce in reference to the former, would be concentrated in the equatorial regions. The mountains are more numerous and higher at low latitudes than at high latitudes. The only way that this can be attributed to decreasing rotation is to suppose that the mountain-making localities were determined by the changes due to these causes, and that subsequent deformations have continued along the old zones of weakness.

However, decreasing oblateness and increasing pressures are available to explain the great deformation of the older rocks, and especially those of the Archean and Algonkian eras.

The amount of contraction which can be attributed to loss of heat is also a steadily decreasing quantity. However, in vulcanism we find a cause for crustal corrugation perhaps as potent now as at any time since the beginning of the Algonkian. Indeed, as has been seen (p. 48), the greatest volcanic epoch of which we have certain knowledge is late Tertiary time, and contemporaneous with this was the great Tertiary period of mountain-making.

It is clear that the explanation offered for crustal deformation is complex. The theory is a combined contractional and transfer theory. Moreover, the contraction, instead of being assigned to a single cause, secular cooling, is assigned to this and to increased pressure and changing physical condition. Also

[1] On the secular acceleration of the mean movement of the moon, by ARTHUR CAYLEY: Monthly Notices, Roy. Astr. Soc., Vol. XXII, 1862, pp. 171–230.

the transfers of material are of several kinds, but those of vulcanism and those of changing oblateness are the more important. The conclusions reached may therefore be considered as illustrating Chamberlin's method of multiple working hypotheses.

It is to be noted in conclusion that the argument of the above paper is independent of any theory of the origin of the earth, and of any theory of the condition of its interior, provided it is largely limited in its application to the time since the earth in some way had attained approximately its present mass. Furthermore, the contractional and corrugating effects dependent upon changing rotation involve the hypothesis that at one time the earth rotated upon its axis several times more rapidly than at present. If a more rapid rotational period be assumed than that discussed, the resultant effects would be correspondingly greater. But however the earth originated, and whatever the condition of the interior, the considerations offered which should be taken into account in estimates of crustal shortening are applicable.

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